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Comment on nhess-2022-21

Anonymous Referee #2

Referee comment on "Using Sentinel-1 radar amplitude time series to constrain the timings of individual landslides: a step towards understanding the controls on monsoon-triggered landsliding" by Katy Burrows et al., Nat. Hazards Earth Syst. Sci. Discuss., <https://doi.org/10.5194/nhess-2022-21-RC2>, 2022

In this paper, the authors propose a SAR-based technique to estimate the possible time-window of landslides mapped as a part of seasonally generated inventories. To test their methods, they use two rainfall-triggered landslide event inventories and one post-seismic inventory including landslides that might have been triggered by the aftershocks of the 2015 Gorkha earthquake and/or rainfall events that occurred following the event. In this context, I should stress that the paper focuses on an interesting research question for sure and it appears as a nice fit for the journal and, in particular, for this special issue.

However, the authors are able to come up with a time estimation only for 20% of landslides with an accuracy of 80%. Therefore, I doubt if this is successful research in the end. Frankly speaking, I am not sure and just hesitating to say that the results are promising. However, what I can say is the output of this research is not fulfilling what the authors are promising in the abstract/conclusions.

This being said, one could consider this paper as a step towards developing better tools along this research direction and in this regard, could be still valuable. And yet, authors do not clearly present their work. Unfortunately, the manuscript is not well written. I had to read some parts more than once to understand the authors' point. The figures are not well designed either. I have many comments that I hope the authors find useful to improve their work.

Last but not least, I would like to test the code/tool they developed but unfortunately, it is not available. This is a preprint with DOI, so I did not really get why it was not shared already.

Overall, I recommend a rejection to give adequate time to the authors for a

comprehensive revision for the manuscript for clarity, pulling some of the speculation and assumptions to the discussion, adding more definitions of terms, and framing the paper in hypotheses. This should help the reader understand what you did and why you did it. Because in the current version, authors do not really help the reader to find their way through the manuscript.

Below I've included line-by-line suggestions and highlighted all these points.

Line 18: "to emergency response coordinators". I would say rainfall-induced landslide inventories are rarely used by emergency response coordinators as they are generated at least weeks or months after an event. But if you are referring to some kind of indirect usage of the dataset (for instance, as an input to develop a landslide early warning system or something) please be more specific.

Line 18: "physical and empirical" there are also statistically-based models exploiting the very same dataset

Lines 18-19: Could you please cite relevant literature.

Line 20: "the size location" the size, location

Line 22: "the size and location" the size, location and timing too. As you said occurrence dates of landslides could not be accurate in some cases via optical images but also, as you said, if we have cloud-free images it is doable.

Line 24: "Williams et al., 2018; Robinson et al., 2019". These examples are from earthquake-triggered landslide events. But you focus on rainfall-triggered landslides. So, please replace them with some examples of rainfall-triggered landslide events.

Lines 28-29: This line needs to be rewritten. Also, why did you prefer the term "landfall", why not "landslide"

Lines 30-31: Is that the case? Landslides triggered by each of those typhoons were mapped separately or not? It is not clear from the line if you indicate what already happened or this is just a hypothetical remark.

Lines 33-34: "This limits analysis of these landslides to the annual scale (e.g. Marc et al., 2019a; Jones et al., 2021)." But, for instance, Marc and others generated monsoon-induced landslide inventories and to do that you just need pre- and post- monsoon images. So you do not need cloud-free optical satellite images through the monsoon. Please remove this reference and also please be more specific about the limitations of generating seasonal landslide inventories.

Line 35: "Current alternative methods of landslide timing are generally not widely applicable." Please rewrite this line, is not clear what you mean. What are those alternative methods? And why do you think they are not widely applicable (any reference for this?). You haven't said anything about any alternative methods yet. Please first describe them and then you can evaluate those methods based on the literature.

Line 36: But this is not the method Kirshbaum and others or if you take a look at more recent literature Franceschini and others (DOI 10.1007/s10346-021-01799-y) used, this is the source information for them. Please tell us the method they used.

Line 43: "will" Why did you switch to the future tense

Line 49. Please put a full stop before giving the example.

Line 57: "timed landslide information" this is the first time that I have heard this term and it sounds weird, please rephrase it. And please do it not only here but through the manuscript.

Line 61: "three potential landslide timing methods" you haven't said anything about these methods yet, so it is not clear what these methods are.

Line 63: "Case study events" does not sound right. Please revise it. e.g., Case studies or Landslide inventories

Line 66: Why did you take 20 pixels as your threshold? Why not 10? It could be better to

do it without any filtering first. And then, you can identify the threshold for the landslide size that your method works well.

Line 68: "inventories of landslides" landslide inventories. Btw, you do not really need to cite Emberson et al. (2021) for the Hiroshima inventory because it was already available, right?

Line 70: "heavy rainfall event which took place from 28 June to 9 July 2018" I guess these are the dates that they were able to acquire pre- and post- event images to map landslides, right? But was the study area also exposed to heavy precipitation during the entire period? It would be useful to see the amount of precipitation (as time series) that each of your study areas received during the periods under consideration.

Line 74: "Planetdove" Planet Scope?

Lines 74-75: "images acquired on 20 and 24 March...the majority of landsliding occurred between the 15-17 March" You mean, they did not examine pre and post images to identify landslides solely triggered by the rainfall event, is this correct?

Line 77: I thought you focus on rainfall-triggered landslides as also you indicated in the title of your manuscript. Why do you use the co-seismic landslide inventory of Roback et al. (2018)?

Ok, now I understand what you have done. You removed landslides triggered by the mainshock and work with the others. However, you do not know if these landslides were triggered by the aftershocks or rainfall events. Also, it is not clear when they were triggered. The confusing thing is you mentioned that you focus on "inventories of landslides whose timings are known a-priori to test and develop landslide". However, this does not one of those inventories. Why do not you simply pick another rainfall-induced landslide event inventory?

65 timing methods.

Line 81: "we used earthquake-triggered landslides, which can be assumed to occur concurrently with the ground shaking" you already said it above, please remove this.

Lines 81-83: "since the inventory of Roback et al. (2018) covers a large area, with different areas having different Sentinel-1 coverage, we focused on triggered landslides within three large valleys" This does not explain why you focus on these three rectangular areas (actually one of them has a weird shape). You can cover a larger area mapped by Roback et al. (2018) and one Sentinel 1 image should be covering at least an area covering two of those rectangles. So how did you identify these rectangles really?

Lines 82-83: "large area & large valleys" please be more specific; either tell it directly or not mention it at all.

Line 83: "valleys see large numbers of rainfall-triggered landslides" it does not sound correct, please fix the language.

Line 84: "the timing of which would be one of the key applications of our method" Please remove this line. You already indicated your motivation.

Line 89: You are using the inventory mapped by Roback and others but for some reason, you are citing Marc and others. Too much self-citation, remove Marc et al. (2019)

Line 90: "close enough" not clear what this means. What would be close enough? What were the PGA or PGV values at those valleys? Did Martha and others map no landslide at those valleys and say this based on their observations? Or is this just an interpretation?

Figure 1: If you have such a plot (i.e., panel d) then please indicate ascending and descending images in panel d. This will be specifically important to see the dataset you used in the Gorkha case where some of the ascending images should be missing. Please properly indicate what those abbreviations stand for (e.g., H in panel a and so on). It is not clear how did you define pre-, co- and post- event image acquisitions. Do you explain this later in the method section? But you already refer to the term "co-event pair" in line 91, so the reader needs to know what it means. For instance, you indicate that in the Hiroshima case the heavy rainfall event occurred between 28 June and 9 July. Therefore, landslides were triggered (or mapped, as I mentioned this is confusing anyway) during this 10-day period. And you also mention that landslides were most likely triggered between 6 and 7th of July. Then why do you have such a large time period for "co-event pair"

Also, why do you represent the real event date as a single day? You mentioned above some time slots that landslides were most likely triggered. Why do not you indicate them also in panel d?

Line 92: "these two earthquakes can be considered as a single triggering event in Bhote Kosi" You can not consider them as a single event. However, if you cannot differentiate landslides possibly triggered by different factors for a given period of time, this needs to be indicated as a source of uncertainty in your analyses. I do not know what could be the consequences, but obviously, this needs to be discussed later on in the manuscript.

Lines 131-133: Could please explain how you defined these time windows (i.e, 6, 3 and 2 months)? What is the logic behind it?

Line 131: "approximately six months" Later on you are saying two cases you took it as 6 months and in another one like 5 months. No need to repeat the same things. Please remove "approximately six months"

Line 139: "In this figure" Which figure? Figure 1d? Then say it, please.

Line 141: "ascending track 72 over Zimbabwe will be referred to as Z072A" this is not a good idea. Why don't you refer to it, for instance, as Zimbabwe-asc or Z-acs. Or something like we can easily understand what you are referring to.

Line 143: Please make a kind of introduction and tell us that you will introduce three different methods for some reason. And please indicate that reason too. It is difficult to follow the text. You are explaining your method (which is ok, I do not have any complaints) but if we do not understand why you are providing this information, we cannot follow you.

Line 150: "pixels that are dissimilar to those within the landslide, for example pixels located on the opposite side of a ridge, in a river or with different surface cover" Could you be more specific? How do you define similar and dissimilar pixels? Based on what? Based on land cover? Or do you have some other criteria you take into account?

I see, in the next lines you are explaining those variables. But please first tell us what we are talking about (i.e., what you mean by dissimilar pixels) and then you can mention that you removed them.

Line 151: "three surfaces" three variables might be better

Line 154: "amplitude variability" is this the third one? You mentioned the first and second variables but which one is the third?

Figure 2: Please fix the label of the panel (c) and please also indicate the label (c). Remove label (b) from panel (a).

What do you mean by "vegetation removed"? Do you mean because of landsliding? If it is the case, no need to indicate this.

Is the blue bar not centered for some reason? Did you do this on purpose? Or is this something you need to fix? And please indicate the corresponding panels while referring to "blue bars".

Line 160: "this"?

Line 164: "When combining methods, we found" This is still your method section and you haven't said anything about other methods yet. This is to say that I do not understand what you are referring to?

Lines 160-166: Based on what you explained here how we should interpret Figure 2c? "A step change in the difference between the median landslide amplitude and the median background amplitude is then used as an indicator of landslide timing." Based on your interpretation, could you point out the timing of the landslide in Figure 2c. Which one is a step increase or a step decrease? Other than the signal received from the shadow area, I do not see any significant change in overall fluctuations of amplitude values associated with rainfall events (indicated by the blue bar in fig2c).

Lines 168-171: The same comment as above, please explain how you interpret Figure 2d. I do not see a specific change in the trend associated with the blue bar other than some fluctuations.

Lines 185-188: I can not see any connections between these two lines. Could you be more clear about what you mentioned about uncertainty in landslide mapping in the first line?

Line 196: "Step change identification" As usual, please help the reader to follow you. You

have just mentioned three methods to identify the timing of landslides. And I guess you are going to combine these three methods to get the best result out of all, right? This is also not clear and needs to be indicated. And here you keep going with another step of your methodology. I think it would be great if you make a flow chart explaining your methodology. You can briefly describe each and every step of your method at the beginning and then we would have an idea about what is going to be in the next step. I know what I am suggesting is a super smart thing, is quite a traditional way of presenting your method but it is also a good way of doing this.

Line 197: "Sects. 2.4.2, 2.4.1 and 2.4.3" just say above

Line 198: "The step function was made up of a series of -1s and 1s of twice the length of the co-event time series" Do twice the length of the co-event time-series means 12 months? And why?

Can't you make a figure to explain what you have done at this step?

Lines 216-217: "the correct date by chance for a method with no skill" is not clear!

Table 2: What do those percentages stand for? For instance, in Hiroshima (H083A), you have 540 landslides and based on Pixel Variability you correctly identified the occurrence dates of 181 landslides, right? This means you correctly identified 33% of them. Then where did 59% come from? Obviously, the percentages indicate something else but I did not get what it is. I am sorry maybe it is my fault that I could not get it but this is not clear for sure.

Actually based on these numbers and what you present in Figure 5, you can make an estimation for only a small fraction of the examined landslide population, right?

You mentioned about confusion matrix, then why don't you present your results based on that structure?

Lines 243-245: "Out of all the non-masked landslides in each inventory, 23% were assigned a date in Hiroshima, 21% in Zimbabwe and 14% in Trishuli and of these, 80% of the estimated dates in Hiroshima were correct, 73% in Zimbabwe and 81% in Trishuli (Table 2)." So as you also indicated in the abstract, these are the percentages of correctly predicted landslides:

Hiroshima ~18%

Zimbabwe~15%

Trishuli~11%

Then how about the rest? Then I do not think what you mentioned in your abstract is convincing:

"This will allow multi-temporal landslide inventories to be generated for long rainfall events such as the Indian summer monsoon, which triggers large numbers of landslides every year and has until now been limited to annual-scale analysis."

Landslides could occur on different dates over a monsoon season in a given area of interest. And we would miss a great majority of them if we use this technique. Therefore, I do not think we can confidently argue that multi-temporal inventories can be generated based on this method (This method does not mean to generate multi-temporal inventories anyway). I am not saying this method is useless but is also clear that this could be just a small step towards what you are arguing in your abstract.

Line 250: "Factors affecting performance of each method" This does not sound like your results. You should move this section to the Discussion section.

Line 383: " Application to future events" Please merge this section with the conclusion section, no need to have this heading, the paper is already too long.

Line 384: As you said you are just estimating the time window that landslide might have occurred. So you are not estimating the exact occurrence date of landslides. You should clarify this also in your title.

Line 406: "generate multi-temporal" You are not generating landslide inventories. You are just trying to label existing landslide inventory in terms of their time of occurrences.

Lines 414-415: "Google Earth Engine and Python codes used in generating the time series and detecting landslide timings will be provided if the manuscript is accepted for publication" The authors should share the code so we can check how it works really.

